With the Author's Compliments

ON THE EPIDEMIOLOGY OF PLAGUE.

BY

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Late Fellow of St John's College, Cambridge, Fellow of Allahabad University, Chemical Examiner and Bacteriologist to the United Provinces and to the Central Provinces.

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(*From the Government Laboratory, Agra, India.*)

Characters of areas in which plague is endemic.

In areas in which plague is present, or has recently been present, in India, as an epidemic, no definite relation has been observed between intensity of plague (apart from its persistence), and badness of sanitary condition of dwellings[1]. On the other hand, in areas in which plague is present endemically, so far as evidence goes, very unsatisfactory sanitary conditions exist. Such areas are generally situated in mountainous countries (Garhwal, Yunnan, Beni-Cheir, Transbaikalia). The inhabitants of such places, owing to the difficulty of obtaining water for domestic purposes, are apt to be filthy both in their houses and persons. In Garhwal, Yunnan, and Beni-Cheir

[1] In Bombay only 8·6 per cent. of tenements condemned as unfit for human habitation in 1897 were situated in wards F and G, which were the portions of the town most severely attacked by plague. For other facts of the same nature see Hankin, "La propagation de la Peste" (*Annales de l'Institut Pasteur*, November, 1898, p. 705). Dr Weir, the Health Officer of Bombay in 1896, stated that the houses in the suburban villages near Bombay which suffered severely from plague were not so overcrowded as the houses in the slums of the city which were far less severely attacked. (*Evidence before the Indian Plague Commission*, Vol. III. p. 311.) Mr Winter, who was in charge of the plague operations in Jawalapur, stated that more cases of plague occurred in large well-built houses than in mud huts, at any rate at the commencement of the outbreak. In a later paragraph it will be pointed out that a large proportion of the earlier cases in Jawalapur were of grain dealers, that is to say of persons who, though well-to-do and living usually in well-built houses, were likely to come into places infested with rats. (*Evidence before the Indian Plague Commission*, Vol. II. p. 52.)

domesticated animals are stabled in the houses, the paucity of level area making separate provision for cattle a matter of difficulty. Stable refuse consequently is liable to accumulate in the lower floors of the houses, producing conditions favourable to the presence of swarms of fleas.

As illustrating the above statement, the following description of an infected house in the endemic area of Garhwal is quoted from Planck[1]:

"The infected house was found to be an old, roughly-built, thatched double tenement, placed on a solitary ledge. The site generally, filthy from manure and overgrown with excessive vegetation. The lower rooms had long been used as cattle or goat pens, the upper had apparently never been cleansed or washed since the house was built, and were infested with hungry fleas in such extraordinary numbers that a few minutes' stay on the premises necessitated an immediate resort to a neighbouring stream, there to await the arrival of a change of clothing. The two native officials who assisted at this inspection were attacked in like manner, and a few minutes after the house had been hastily fired all three persons were immersed in the pool below it."

In the Mesopotamian area in which plague appears to be endemic, I am informed by Mr Syad Abulhasun, Tehsildar of Agra, who has travelled in that country, that the villages are infested with large numbers of fleas. He states that though the inhabitants are ignorant and uncivilised they instantly vacate and burn down their villages on the appearance of plague.

Up to the present time, endemic areas have been excluded from any great amount of human traffic or intercourse, either owing to the paucity of their inhabitants or to the nature of the country. Probably this is the reason why they have not, more often than has been the case, served as sources of outbreaks of plague. But we cannot assert that this state of things will continue to exist in the future. The Trans-Siberian Railway may open up the Transbaikalian and Mongolian endemic areas; the Cape to Cairo Railway that of Uganda. A projected railway from Burmah to China might, if carried out, give further opportunities of acquaintance with the endemic plague of Yunnan, and a proposed railway along the Euphrates valley may do the same for the infected area of Mesopotamia. Thus plague, far from being a disease that promises to become extinct, threatens to be an increased source of anxiety.

[1] *Annual Report* of the Sanitary Commissioner for the North-West Provinces and Oudh for the year 1876.

Persistence of plague in endemic areas.

A study of the plague in the Indian endemic area[1] of Garhwal indicates that it does not persist there owing to a constant succession of cases of the disease. On the contrary the disease remains apparently extinct for years and then suddenly breaks out in fully virulent form. The observations of Planck[2] leave little room for doubt that during the intervals between successive outbreaks the microbe does not continue to exist owing to a succession of passages either through human beings or through rats. Judging from present evidence it would appear that it exists in a latent state so far as these particular living beings are concerned.

Indian plague outbreaks and wanderings of fakirs.

The following table[3] gives a complete list of recorded plague outbreaks in India proper, and also of outbreaks in the endemic area of Garhwal, including Kumaon. Garhwal is a mountainous and somewhat inaccessible country, having but little ordinary traffic with the rest of India. Only on one occasion (namely, a small outbreak at Moradabad in 1853) is it likely that this ordinary traffic has resulted in an exportation of the disease. Two sacred shrines are present that are annually visited by pilgrims from the plains of India, but no case is known of these pilgrims having been concerned in the transmission of the disease. But there are in Garhwal numbers of strict ascetics or fakirs who usually reside at or near the sacred shrines. On certain occasions these fakirs travel through Garhwal and the plains of India to places of pilgrimage where from time immemorial festivals have been held at twelve-yearly intervals. It was originally suggested by the German Bombay Plague Commission that fakirs from Garhwal were the source of the Bombay plague. The facts included in the following table strongly suggest that most other known outbreaks of Indian plague have a similar origin.

[1] The phrase "endemic area" will, I hope, be regarded as an allowable contraction for the phrase "area in which the disease is endemic." Convenience may justify a use of the word not implied by its derivation.

[2] *loc. cit.*

[3] For facts included in this table I am chiefly indebted to "The Plague in India" (*Official Report*, compiled by R. Nathan, Indian Civil Service), and to Hutcheson, "Mahamari" (*Transactions* of the Indian Medical Congress held at Calcutta in December 1894, p. 304).

Date	Pilgrim festivals visited by Garhwali fakirs, held at twelve-yearly intervals at :—	Plague in endemic area of Garhwal including Kumaon	Plague in other parts of India
1344	Nassik		Army of Sultan Mahommed Tughlak destroyed by pestilence probably near Deogiri a town at a short distance from Nassik.
1608	Nassik		
1611	... ...		Plague said to have commenced in Punjab. Lasted 7 years, and spread to Delhi, Agra, Cashmere, and Kandahar.
1680	Nassik		
1683	... ...		Confined to Western India. Lasted in Ahmedabad for 8 years.
1812	Nassik		Began in Gujerat and lasted 9 years.
1822	Allahabad	First recorded appearance of plague in Garhwal	
1823		A few villages attacked	
1824	Nassik	0	
1825	0	0	
1826	0	0	
1827	0	0	
1828	0	0	
1829	0	0	
1830	0	0	
1831	0	0	
1832	0	0	
1833	0	0	
1834	Allahabad	Severe	
1835	0	Severe	
1836	Nassik	Severe	The Pali Plague commenced at village of Taiwali near Pali. Stated by villagers to have been brought by wandering fakirs. Lasted 2 years.
1837	0	Severe in one district	
1838	0	0	
1839	0	0	
1840	0	0	
1841	0	0	
1842	0	0	
1843	0	0	
1844	0	0	
1845	0	0	
1846	Allahabad	At source of Ramgunga and nearly depopulated Sarkot	
1847	0	One village	

Date	Pilgrim festivals visited by Garhwali fakirs, held at twelve-yearly intervals at :—	Plague in endemic area of Garhwal including Kumaon	Plague in other parts of India
1848	Nassik	A few villages	
1849	0	Virulent in 2 villages	
1850	0	One village	
1851	0	Several villages	
1852	0	Some villages	
1853	0	0	In Moradabad, said to have been brought from Garhwal.
1854	0	0	
1855	0	0	
1856	0	0	
1857	0	0	
1858	Allahabad	0	
1859	0	Began in northern districts	
1860	Nassik	Severe	
1861	0	0	
1862	0	0	
1863	0	0	
1864	0	0	
1865	0	0	
1866	0	0	
1867	0	0	
1868	0	0	
1869	0	0	
1870	Allahabad	One village	
1871	0	0	
1872	Nassik	0	
1873	0	0	
1874	0	0	
1875	0	6 cases	
1876	0	327 deaths	
1877	0	535 deaths	
1878	0	10 deaths	
1879	0	0	
1880	0	0	
1881	0	0	
1882	Allahabad	0	
1883	0	0	
1884	Nassik	Severe	
1885	0	?	
1886	0	"A great number of deaths"	
1887	0	82 deaths	
1888	0	About 36 deaths	
1889	0	0	
1890	0	0	
1891	0	Six deaths	
1892	0	0	
1893	0	Two villages, 16 attacks	

Date	Pilgrim festivals visited by Garhwali fakirs, held at twelve-yearly intervals at:—	Plague in endemic area of Garhwal including Kumaon	Plague in other parts of India
1894	Allahabad	Probably two villages	
1895	0	0	
1896	Nassik	0	Plague commenced in Bombay.
1897	0	One village, 17 attacks	
1898	0	0	
1899	0	0	
1900	0	0	

(*Note.* According to Hutcheson the history of the disease in Garhwal from 1887 up to 1894 is imperfect, owing to the view having been adopted that it was identical with typhus fever.)

Plague often carried by persons not themselves infected at the time.

In view of the evidence now accumulated from the history of the Bombay outbreak, there can be no doubt that plague is, not infrequently, carried from place to place by persons who themselves escape the disease or who are not the first attacked in the places to which they have carried the infection. This curious fact is however not simply a feature of the Bombay plague. It was noticed both in the plague of Justinian and during the Black Death. Consequently the suggestion that Garhwali fakirs have been concerned with the spread of Indian plague does not imply that the fakirs themselves must in so doing have suffered from the disease.

Derivation of Bombay plague, probably Garhwal.

The question of the origin of the Bombay plague must now be considered. The first cases of disease recognised as plague occurred on the 10th of August, 1896. As to the source of this infection nothing was definitely known at the time. It was first supposed that plague was brought from Hong Kong, but no evidence exists that dock labourers or travellers from Hong Kong were among the first affected. So far as the evidence goes, those first attacked were principally grain dealers and the rats that haunted their shops, but not the rats that found a living in the docks. As already mentioned an alternative suggestion was put forward in the report of the German Bombay Plague Commission to the effect that the plague was imported into Bombay by fakirs from Garhwal.

The known facts bearing on the case are as follows : In July and August, 1896, about two thousand fakirs came to Bombay from northern India on their way to a religious festival at Nassik. They encamped for the most part in and near the temple compound of Walkeshwar in Bombay. But during the day they spent their time begging among the Bunnias (grain dealers) and Bhattias, in Mandvie, the district of the city where the plague first appeared. The attention of the Commissioner of Police was drawn to these fakirs owing to the primeval simplicity of their costume. They hotly protested against interference, saying that they always wore the same clothing in the Himalaya Mountains from whence they had come. On being asked in what part of the Himalayas they lived they replied Garhwal. At least four of them gave this reply. They somewhat outstayed their welcome, and during August the Bunnias of Mandvie combined together and paid the railway fares of 800 of them who had not previously started, to Nassik, where the festival was to commence on the 13th of that month. At its commencement the outbreak of plague, that began about this time, was almost wholly confined to the Bhattia and Bunnia communities who had been especially in contact with the Garhwali fakirs. In the week ending the 25th August three suspicious deaths from fever and pneumonia occurred in Walkeshwar where the fakirs resided. Of these one was a servant and two were "mendicants." These reasons for suspecting the fakirs to have imported the infection are to be found in the Report of the German Plague Commission.

Probable similar origin for earlier Indian outbreaks.

Another consideration, it appears to me, can be put forward bearing on the question. The origin of the outbreaks of plague that occurred in western India, in the years 1812 and 1836, needs to be explained as much as the origin of the Bombay outbreak of 1896. On the two former occasions there was no plague in Hong Kong. Neither did these Indian outbreaks commence in sea-ports, which had it been the case might have been a ground for suspecting an importation from abroad. Can these two former outbreaks have been due to the wanderings of fakirs? The Garhwali fakirs only visit western India on the occasion of the Nassik festival, which is held at twelve-yearly intervals. Consequently if the former Indian outbreaks of plague had been caused by Garhwali fakirs, they should have occurred in years in which the Nassik festival was held. The year 1896, the date of the Bombay plague, was such an occasion. Counting backwards, by twelve-yearly intervals, we find that the Nassik festival was held in 1836, the year of the Pali plague, and in the year 1812, the year of the Gujerat outbreak. It is at least a striking coincidence that of the eight twelve-yearly festivals held during the nineteenth century at Nassik, three should have coincided with outbreaks of plague, and these the only outbreaks that happened.

The Pali plague.

In the case of the Pali plague I have been so fortunate as to find some evidence connecting its origin with wandering fakirs. The disease is generally supposed to have commenced in the town of Pali in July 1836 among the calico printers, a caste numbering about 2000 persons, of whom 655 succumbed to the disease. But in the *Bombay Medical and Physical Society's Transactions for* 1839 (Vol. II., p. 1) is a report, by Forbes, an Assistant Surgeon, on the Pali plague. He states that there is no doubt that the disease originated in the village of Taiwali, "ten short koss" south-east of Pali, in the month of April, and that from thence it was brought into the town. According to the statements of the natives, a party of wandering fakirs (Gosains) "on their return from Dwarka in Kathiawar, halted near the village, close to some fields, and began to carry off dry thorns from the hedges to cook their food with; the Gosains fled, leaving their malediction on the men, who soon after sickened and died of this uncommon illness." In another publication[1] on the subject he says, "The most singular phenomenon remarked in connection with the breaking out of the disease, and adverted to in Mr White's report, was the death of all the rats of the village of Taiwali, during the latter half of April, and just before its first appearance. They lay dead in all places and directions, in the streets, houses, and hiding-places of the walls," and that "this death of the animal attended or preceded the disease in every town that was attacked in Marwar, so that the inhabitants of any house instantly quitted it on seeing a dead rat."

Thinking that the statement that the fakirs in Bombay had come from so distant a country as Garhwal needed some further confirmation, I made enquiries as to the habits of these fakirs from several inhabitants of Garhwal during a visit to Naini Tal in the Himalaya Mountains. I was also so fortunate as to meet a fakir who had been to Nassik from Garhwal on the occasion of the festival of 1896, as well as on previous occasions. He assured me that Garhwali fakirs never travel to the Bombay Presidency except on the occasion of these twelve-yearly Nassik festivals. They travel by different routes. His route, which I took down from his dictation, was as follows: Gangotri (in Garhwal), Hardwar, Gorakhpur, Patna, Rewa, Banda, Chitrakot, Jhansi, Indore, near the Nerbudda river, Bhusawal, Jalgaon, Dhoolia, Punchbhatti, Tirmook, Nassik. This measured on the map gives a distance of about 2,150 miles, which he had travelled in a little over five months, giving an average of about 14 miles a day. Other fakirs travelled, probably by more direct

[1] *Thesis on the Nature and History of Plague as observed in the North-Western Provinces of India* (published in Edinburgh in 1840 by Machlachan Stewart and Co.), p. 34.

routes, to Bombay, and returned thence by rail to Nassik. The statement that Garhwali fakirs only visit the Bombay Presidency on the occasion of the Nassik festivals is obviously of importance in this connection.

Characters of Bombay plague.

The plague in the Bombay Presidency differs from plague at present existing outside India in other important respects besides in its greater persistency and intensity. Firstly, it has shown its maximum virulence not in towns but in villages[1]. Secondly, though it spreads with facility from a town or village to a neighbouring village, it does not appear often to be carried to great distances in epidemic form. Further, in India, it shows no tendency to spread along trade routes as such.

With regard to the greater virulence of Indian plague in small towns and villages than in large towns, the cases of Bombay and Oporto may be cited and compared. In both cases there was panic among the inhabitants on the appearance of the disease, and in both cases the infection was carried into towns and villages in the neighbourhood. In the case of Bombay the outbreaks thus produced often exceeded greatly in virulence the original outbreak in that city. In the case of Oporto though isolated cases of plague occurred in many villages in the neighbourhood, in no case did the disease succeed in establishing itself in epidemic form outside the town. Similarly in Tamatave, the disease spread to the surrounding country, but appears to have died out there before it came to an end in the town itself. Generally it may be stated, that, so far as existing evidence goes, plague, in the present outbreak, outside India has attached itself especially to the crowded and insanitary portions of large towns.

Similar characters possessed by other Indian plagues.

Can we find among other outbreaks an analogy for the peculiar characters of the present Indian plague?

[1] The intensity of plague in towns and villages in the Bombay Presidency has been in inverse proportion to their size as exemplified by the following table, which relates to certain plague outbreaks occurring between 1897 and 1898 in that Presidency:—

Name of place	Number of inhabitants	Death rate from plague per 1000 inhabitants
Bombay	806,144	20·1
Poona	161,696	31·2
Karachi	97,009	24·1
Sholapur	61,564	35·0
Kale	4,431	104·9
Supne	2,068	102·5
Ibrampur	1,692	360·5

The death rate of Bombay for 1896–97 is derived from lower plague death rates from the crowded and less sanitary central portions of the city, and higher plague death rates from suburban districts where village conditions prevailed.

On referring to records of the plague outbreak in Western India in 1812, it appears that, so far as the evidence goes, it resembled the present Indian plague, both in its avoidance of trade routes as such, and in its great virulence in villages as compared with towns, during the first eight years of its existence[1]. It was only at about the end of this period that it showed itself as a virulent disease in a comparatively large town (Ahmedabad). Another plague having similar characters also occurred in Western India, namely, the Pali plague of 1836. Though this produced a high mortality in the small town of Pali, it appears to have been more destructive in the surrounding country, where in spite of the smallness of the area affected, it is variously estimated to have destroyed from 60,000 to 100,000 persons. Pali was then an important trade centre, yet the disease failed to spread along any trade routes from this town.

The fact that these three plagues in Western India have these characters in common harmonises with the suggestion that they have a common origin.

Resemblances of Black Death to Indian plague.

A more important outbreak that appears to have resembled the Bombay plague in its great intensity, its virulence in villages, and in other characters, is the Black Death, and to some extent the European outbreaks that immediately followed it[2]. According to Simon von Covino, who observed the Black Death in Paris, Montpellier, and probably other places, it was especially severe in the smaller towns (*suburbia*). Creighton[3] states that probably two-thirds of the country clergy were destroyed by this pestilence in England. He adds, "This alone would suffice to show that the virus of the Black Death permeated the soil everywhere, country and town alike. It is this universality of incidence that chiefly distinguishes the Black Death from the later outbreaks of plague, which were more often in towns than in villages or in scattered houses, and were seldom in many places in the same year." Elsewhere the same author states that the later outbreaks of plague in England resembled the Black Death in being "universal and in the homes of the peasantry" until 1407 or perhaps

[1] Nathan, *loc. cit.*

[2] See Haeser, *Geschichte der Medicin und der epidemischen Krankheiten*, pp. 142 and 170.

[3] *History of Epidemics in Britain* (Cambridge, 1891).

1439. "From that time onwards town and country are contrasted in the matter of plague; it became usual to flee to the country so as to escape the pestilential air in town in the summer heats[1]." Creighton also quotes many instances in which the mortality was so great in the country that no heirs existed to inherit estates, or peasants to till the ground. No evidence of such depopulation exists for London or other towns.

Thus all the known plagues of Western India resemble the Black Death and the epidemics to which it gave rise, in showing a high degree of intensity at one time over a large area, and in the relatively high rate of mortality that they produced in villages as compared with towns. The two groups of outbreaks also resemble one another in their power of spreading with facility from village to village. With regard to the extent of area attacked, we find, it must be admitted, a contrast. But in this respect the present Bombay plague may be regarded as intermediate between the Black Death and the Pali plague. Correlated with the high degree of virulence, there appears to be a certain similarity in the symptoms observed in these outbreaks. When some years ago a controversy arose as to whether Levantine plague was the same disease as the Black Death, the matter was settled by reference to the plagues of Gujerat and Pali, in which the pneumonic form of the disease had been observed, as happened during the Black Death, and as was not usually the case in plague of Levantine origin in modern times.

From these considerations it follows that some of the arguments that have been relied on above to support the idea that Western Indian plagues have a common origin in Garhwal, must fall to the ground unless it can be shown that the Black Death may also have been a result of the wanderings of Garhwali fakirs.

Probable Indian origin of Black Death.

No satisfactory suggestion has as yet been put forward as to the original source of the Black Death. It will therefore be of interest to discuss the possibility of its having come from Garhwal.

More than one contemporary historian relates that the Black Death was brought to Europe from the Crimea. But the exact circumstances of this event were unknown until the discovery in 1842 by Henschel of a most interesting manuscript in the Rhediger Library at Breslau[2].

[1] *loc. cit.* p. 233.

[2] Haeser, *loc. cit.* Vol. III. p. 157.

The author of this manuscript was Gabriel De Mussis, a notary of Piacenza, who was employed by the merchants trading in the Crimea. At the time in question these merchants were attacked by the Tartars, and besieged in the town of Tana on the river Don. They had to retire from this place to the town of Caffa on the sea-coast. Here they were besieged for nearly three years. Suddenly "the death" broke out in the Tartar host and thousands were daily destroyed "as if arrows from heaven were striking at them and breaking down their pride." The Tartars, hoping to communicate the infection, threw the bodies of the dead into the town by means of their catapults. The disease soon broke out among the besieged and they had to evacuate the town in their ships.

Constantinople was the first port reached by the fugitives, and their arrival was shortly after followed by the appearance of the disease. The Emperor, John Cantacuzenes[1], wrote an account of the pestilence which is still extant, and in which he states that it came from among the Tartars in the Crimea.

After leaving Constantinople the merchants touched at Messina in Sicily. The consequences of the arrival of the infected ships are thus described by a Franciscan friar, Michael Platiensis:—"A most deadly pestilence sprang up over the entire island. It happened that in the month of October, in the year of our Lord 1347, about the beginning of the month, twelve Genoese ships, flying from the divine vengeance which our Lord for their sins had put upon them, put into the port of Messina, bringing with them such a sickness clinging to their very bones that did anyone speak with them he was directly struck with a mortal sickness from which there was no escape."

Three of the plague-stricken vessels, on one of which was De Mussis, put into Genoa in January. A few days after their arrival the disease appeared in Genoa, although no infection was known at the time to have been present on the ships. It is related that hardly a seventh part of the population of the town was left alive.

The plague was brought to Venice by another of the infected vessels. From this place and from Genoa the disease rapidly spread over the whole of Italy.

As soon as the Genoese authorities recognised that the ships were the source of infection they compelled them to leave the port. One of them is known to have gone to Marseilles, and to have introduced the

[1] Haeser, *loc. cit.* p. 161.

plague into France. In one month the disease is stated to have carried off 57,000 inhabitants of Marseilles and its neighbourhood.

Black Death not derived from China.

In view of the opinion frequently held that the Black Death came from China, it is important to notice that the Chinese annals contain no mention of any such disease in the years preceding the appearance of the Black Death in Europe. On the other hand great plagues are mentioned in the Chinese annals in the years 1352 to 1363[1].

According to De Mussis it commenced, not in the besieged town of Caffa, as one would expect to have been the case had the germ been imported in bales of merchandise from China, but among the surrounding Tartars. In spite of the statements of De Mussis it is probable that ships from other ports in the Black Sea than Caffa also had to do with importing the infection into Europe. The disease seems to have reached both Sicily and Italy in 1346, but was not known to have been present in Constantinople till the spring of 1347.

The plague, according to Ibn Batuta, reached Jerusalem in the spring of 1348, and Damascus only towards the end of July of the same year. Aleppo and Gaza were attacked in June. Hence this evidence indicates that the plague was present among the Tartars in the Crimea before it was present in Syria. That is to say, it had not followed the ordinary mode of origin of Levantine plague.

The Arab historian Aboel Mahasin[2] states that the plague began in Tartary and travelled thence to the Tartars in the Crimea, and then on to Constantinople and Europe, and in another direction to Asia Minor, Syria, and Egypt.

Indian outbreak antecedent to Black Death.

Having thus traced the Black Death back to Tartary, it is necessary to see whether the records of Indian history contain any mention of a pestilence shortly before that time (1346). As we have seen, plagues in Western India have occurred in years of Nassik festivals. Analogy would therefore lead one to suspect that the Black Death, if of Indian origin, should have commenced shortly after one of these twelve-yearly festivals. Counting backwards by twelve-yearly intervals we find

[1] Creighton, *loc. cit.*

[2] Quoted by Des Guignes in *Histoire des Huns.*

that a pilgrimage must have occurred at Nassik in August of 1344, and in view of the great antiquity of Indian religious customs[1], we may safely surmise that in the spring of that year numbers of fakirs emerged from Garhwal, and travelled by various routes through the plains of India to the sacred shrine. But on referring to Indian histories no explicit mention of plague in that year could be found. Elphinstone[2], however, states that a rebellion broke out in Ma'bar in 1341, and that an army sent by the Emperor of Delhi to suppress it was so wasted by the ravages of a pestilence that it was forced to return[3]. This statement seems to imply that the pestilence appeared soon after the rebellion, namely in 1341, and hence could not be connected with the movements of the fakirs in 1344. But a further examination has shown me that the contrary is the case. In a contemporary history, known as the *Tarikh-i Firoz Shahi*[4], it is stated that on receipt of news of the rebellion an army was despatched to Ma'bar. Afterwards the Emperor returned to Delhi for reinforcements. When he again left Delhi, probably in the autumn of the year, a famine was commencing. When the Emperor was still three months' march distant from Ma'bar, according to the traveller Ibn Batuta, pestilence broke out in the army, and the greater part of it perished. The Emperor himself and many nobles were attacked. After halting at Deogiri (a few marches east of Nassik) while suffering from the disease the Emperor returned to Delhi. He travelled through Malwa, by what appears to have been the ordinary route to Ma'bar, and reached Delhi still weak from his illness (*Tarikh-i Firoz Shahi*)[5].

[1] Alberuni, a Persian writer, was born in A.D. 973, and lived at the court of Mahmud of Ghazni. In his description of India he refers to "the Tree of Prayaga...the place where the waters of the Jaun join the Ganges, where the Hindus torment themselves with various kinds of tortures." Prayaga is the place now known as Allahabad. With little doubt his statement refers to the tricks of fakirs at the pilgrim festival that is still held there. See Alberuni's *India* by Dr E. C. Sachau, London, 1888, Vol. I. p. 200.

[2] *History of India*, Fifth Edition, 1866, p. 406.

[3] The word "waba" used to describe this outbreak is translated as "cholera" in Elliot's translation of Ziaud Din Barni. The word is more usually translated as pestilence and is commonly employed for plague. The known comparatively recent origin of the chief cholera deity in the United Provinces is one reason, among others, for doubting whether at the time we are discussing cholera was so prevalent a disease as it has since become. From Ziaud Din Barni's and also from Ibn Batuta's accounts it appears that the Emperor, when attacked, remained ill from the pestilence for some months, a fact that agrees better with the view that the malady was plague, and not cholera.

[4] By Ziaud Din Barni. See a translation in Elliot's *History of India by its own Historians*, Vol. III. p. 243.

[5] *loc. cit.* p. 244.

When he arrived the famine was at its height. According to the contemporary historian, Ziaud Din Barni, "Not a thousandth part of the population remained. He found the country desolate, a deadly famine raging, and all cultivation abandoned...and man was devouring man." According to Elphinstone, the date of this famine was 1344, and as shown by the above extracts it must have occurred in the same year as the pestilence in the army. Further the year 1344 is given as the date at which the rebels in Ma'bar succeeded in throwing off the authority of the Emperor, and establishing an independent kingdom. This also harmonises with the date given for the destruction of the Emperor's army. Thus this pestilence might have been carried by the commerce in horses and merchandise that then existed to Bokhara and Samarkhand[1] in time to have been the source of the Black Death among the Tartars in 1346[2]. An analogy for so distant a spread of Indian plague appears to exist in the plague of 1611, which commenced in the Punjab and spread to Kashmere and Kandahar.

Thus the resemblance of the Black Death to plagues in Western India does not invalidate the arguments in favour of a common origin of the latter outbreaks based on their epidemiological similarities.

Since writing the above paragraphs I have come across a definite statement by an Arabian author to the effect that the Black Death came from India. Ibn Wady, who is known as a historian by his continuation of the annals of Abulfeda, says that the disease first arose in "the Land of Darkness." Thence it spread to China and India. From India it spread to the land of the Usbeks and Transoxiana, reached Persia, devastated Central Asia, the Crimea, the Byzantine Kingdom, then Cyprus and the Islands. Then the disease reached

[1] Referring to Akbar, Abul Fazl makes the following statements: "His Majesty being very fond of horses, merchants bring them from the two Iraks, Room, Turkestan, Badakshan, Shirvan, Khergez, Tibbet, and Cashmere." (*Ain-I-Akbara*, Gladwin's Translation, Vol. I. p. 130.)

[2] According to Ziaud Din Barni this pestilence broke out at Arangal (spelt by Elphinstone Warangal). According to Ibn Batuta it commenced at Badrakote. In either case it is probable that the infection was brought to the army by reinforcements that had travelled by the ordinary route through Malwa, and crossed the Nerbudda and Tapti rivers, that is to say, who, for some distance, had travelled along the same route as the fakirs on their way to Nassik. The fact that the Emperor halted at Deogiri (on the Godavary below Nassik), and that he was still weak from his illness when he arrived at Delhi three months later, strongly suggests that he was infected not far from the former locality. For the route followed by an army from Delhi that originally conquered Ma'bar in A.D. 1310, see the *Tarikh-i Alai* by Amir Khusru, translated in Elliot's *History*, Vol. III. pp. 86 and 87.

Egypt, desolating Cairo and Alexandria. It extended to Upper Egypt, spread westwards along the African coast to Barka. In the other direction it went through Gaza and Askalon to Syria. It attacked Acca and Jerusalem. At the same time it passed along the coast to Saida and Beyrut. Thence it spread to Damascus. After infecting many other places, which the author mentions, it reached Aleppo, where Ibn Wady himself witnessed its ravages[1].

Means of spread of plague.

It is now necessary to consider the means of spread of the plague infection. Is it by direct contagion from patient to patient? Or from the dejecta of infected rats to human beings? Or does an infected insect play a part in the transmission of the disease?

That the process is not a simple one is indicated by the following considerations.

Simond's latent period in the locality.

When the infectious material has been brought into a village it frequently does not manifest its activity until after a period of weeks or even months, as was first pointed out by Simond for plague in Western India[2]. Both in the Bombay Presidency and in Garhwal the typical mode of development of an outbreak of known history is as follows. The person bringing the infection is, usually, himself attacked, and also a varying number of those in contact with him, within a few days of his arrival, and within the probable incubation period of the disease. The virus then remains quiescent for a long period, generally for about twenty days, but sometimes as little as ten days, and sometimes for a longer period extending to three or more months. The first sign of its renewed activity may be the death of rats, human beings only falling victims after these rodents have been killed off. In other cases rats and men are attacked simultaneously, or lastly only men may be attacked. In the case of Garhwal the phenomena observed by Dr Planck[3] are specially curious. According to this author, outbreaks due to a proved (and recent) importation of the malady are never accompanied by a mortality among rats. On the other hand, "spon-

[1] See Kremer, "Ueber die grossen Seuchen des Orients nach arabischen Quellen," *Sitzungsberichte der Philos. Histor. Cl. d. Kaiserl. Akad. d. Wiss.* Bd. XCVI. p. 69, Vienna, 1880. (I am indebted for this reference to Mr A. W. Thomas.)

[2] "La propagation de la Peste," *Annales de l'Institut Pasteur*, October, 1898, p. 625.

[3] *loc. cit.*

taneous" outbreaks (which may be due to an importation of the virus a long time previously) are often preceded or accompanied by such mortality.

This undeniable fact of the frequent occurrence of a long period of incubation in the locality independently of the incubation period in the human body, indicates that, as a rule, plague spreads, not by simple contagion from patient to patient, but by some deep-seated and perhaps complicated method.

Anomalous outbreaks.

The complexity of the phenomenon is indicated by curious cases in which certain classes of the population or species of animal escape. For instance in Bombay, as I believe is usually the case in plague in India, rats were attacked, while mice escaped. But at Bandora near Bombay, both mice and rats were infected. Plague in Jeddah (probably derived from the Assyr endemic area) in 1897 was accompanied by a mortality of both rats and mice. In other cases, as will be shortly shown, there can be little room for doubt that rats and mice have completely escaped the epidemic[1]. Certain outbreaks appear to have shown a special severity among children.

[1] The outbreak of plague in Kankhal exhibits strikingly the phenomenon of the disease attacking different species of animals at different periods. The infection appears to have been introduced on the 14th May, 1897, by a priest who had been present at the disinfection of a house in Hurdwar on the 7th of May. He died on the 16th May in Kankhal. No further plague was reported till about the 20th June, when an outbreak among rats, bacteriologically diagnosed, occurred in the locality where the first human case had died. The succeeding outbreak among human beings is suspected to have commenced, in the same locality, on the 3rd or 4th August, 1897, though the first case definitely diagnosed occurred on the 6th September. The outbreak among human beings comprised 61 cases, and lasted till the 6th January, 1898. An outbreak of plague among monkeys, bacteriologically diagnosed, began about the middle of October, 1897, and lasted for about a fortnight. Twenty-five dead bodies of monkeys were found, but it is supposed that a larger number were attacked, as when ill these animals are reputed to go into the jungle to die alone. As a precaution about 650 monkeys were caught and kept in cages until the epidemic was at an end. Other monkeys emigrated from Kankhal, and destroyed crops near the village of Jaggitpur, about a mile and a half distant from the town. It was suggested that these monkeys may have been the cause of the epidemic of human plague in that village, that commenced, so far as is known, on the 29th December, 1897, and amounted to 23 cases of the disease. (See *Evidence before the Indian Plague Commission*, of Mr Winter, Vol. II. p. 42, and of Mr Kendall, Vol. II. p. 58.)

"Pestis Puerorum."

The pestilence of 1359 and the following years seems to have marched through a great part of Europe, like the Pied Piper of Hamelin, accompanied, if not as in his case by a disappearance of the rats, at all events by a destruction of children, and is described by more than one contemporary historian as the "pestis puerorum[1]." In Poland it is stated to have attacked chiefly the upper classes and children (1360). In France (1361) it is stated by Guy de Chauliac to have destroyed innumerable children and many members of the upper classes, including five cardinals and a hundred bishops. Two English historians state that the plague of 1361 was known as the "pestis puerorum," but bishops and nobles are recorded to have been among its victims. In a few other exceptional instances the bubonic plague is stated to have shown a tendency to attack children more than adults. For instance the Plague of Justinian appeared in Constantinople in A.D. 542. In A.D. 556 many towns in the Byzantine Empire were again attacked and the disease is stated to have been especially severe among the young. A restricted epidemic in November and December, 1898, and January, 1899, in a suburb of Kurrachi (*i.e.* during the interval between the second and third epidemics in that town) affected 13 children out of a total of 22 attacks. In London in 1382, a disease, probably the bubonic plague, was especially severe among children.

Both in the Black Death and in Indian plague it has been noticed that persons of filthy habits or occupation have escaped the disease. At the commencement of the Bombay outbreak the disease was almost confined to very cleanly castes, whose members as a religious duty never touch food without extensive ablutions. The scavengers remained almost completely exempt from the disease, though their duties must have constantly brought them into contact with the dejecta of plague patients. A similar fact was observed by Dionysius Colle during the Black Death in Italy. He states that "Coriarii qui latrinas exportant,

[1] Since writing this sentence I have had the curiosity to look up the authorities for the story of the Pied Piper. According to Verstegan, the first English writer to describe the incident, it occurred in the year 1376. But according to the brothers Grimm (*Deutsche Sagen*, 1816, Vol. I. pp. 330—333) it happened in the year 1284. According to the story, the Pied Piper appears to have been an indigent person who had witnessed a disappearance of rats in the dominions of the Cham of Cathay whence he had come. After his arrival at the town of Hameln in Brunswick, there was a disappearance first of the rats, and then of the children, the number of the latter being 130. In the following year, according to Schnurrer (*Chronick der Seuchen*) there was so great a mortality in Italy, Lombardy, and Apulia, that many bishops and prelates remained unburied. In 1284 there had been a severe plague outbreak in Egypt. For other authorities see Furnival, *Bibliography of Robert Browning*, pp. 113 and 158. From the facts that tradition associated the event with a particular street in Hameln, and that the archives of the town for some centuries were dated from the time of the disappearance of the children, it appears to be probable that the legend was based on an importation of plague rather than on an ordinary folk-lore tale.

hi etiam, qui xenodochiis inserviunt et locis foetore gravi molestis, omnes fere a peste immunes conspiciebantur; venenum enim venenis debellatur, arcetur, et expellitur[1]."

Rats not a necessary cause or agent in the spread of plague.

Evidence obtained during the Bombay outbreak, as also the evidence from Garhwal, leaves little room for doubt that rats are not a necessary factor in the spread of the plague. In Garhwal out of forty outbreaks investigated by Planck a rat mortality was only observed in eight. This is strong evidence, as the inhabitants knew well the meaning of the sign, and had, in nearly every case, no motive for concealment. In the Bombay Presidency cases occurred in which the same class of observers at one time noticed, and at another time failed to discover the phenomenon. For instance in Hubli, at the time of the commencement of the disease, outside the town near the railway station, numerous dead rats were found. When the disease spread through the town, despite careful search, dead rats were never observed. The Black Death appears to present an analogous phenomenon. When it reached Constantinople it was accompanied by a mortality among rats, but no such phenomenon was recorded during its spread through the rest of Europe[2]. A striking case is that of Kolobouvka in Astrachan, one of the few cases recorded of virulent plague being dealt with successfully. Here 3,000 rodents of different species were examined bacteriologically without the plague microbe being detected in a single case. The officials on the spot ascribed their success in dealing with the outbreak to the fact that it did not extend to rats. The question of the probable relation of rats to the spread of plague will be further discussed in a later paragraph.

[1] Haeser, *loc. cit.* p. 170.

[2] See Abel, "Was wussten unsere Vorfahren von der Empfänglichkeit der Ratten und Mäuse für die Beulenpest des Menschen" (*Zeitschr. f. Hygiene und Infectionskrankheiten*, Vol. xxxvi. p. 89). Abel shows that supposed references to a connection between rats and plague in European medieval authors are for the most part based on a quotation from Avicenna. There is no adequate reason according to Abel for believing that any noticeable rat mortality ever accompanied plague in the Middle Ages in Europe. Eastern authors not infrequently refer to rats staggering about as if drunk in times of plague. In Chaucer's *Canterbury Tales*, in the Knightes Tale (circa A.D. 1380), I noticed the phrase "as dronke as a mous." It would be interesting to know the origin of the expression.

"Contagionist" and "localist" theories of plague.

In a discussion on the probable mode of the spread of the plague virus it will be advisable to consider the bearing of modern discoveries on the controversy between "contagionists" and "localists" as to the nature of the disease.

The "localists" believed that plague was due to a miasma bred on the spot where the disease was manifested. In support of their views they relied on certain facts that tended to show that the disease was not contagious.

The "contagionists," on the other hand, believed that the disease was due to a contagion imported, by means of a plague patient, from some other place where the disease existed. They relied on certain facts that tended to show that the disease could be caught from a patient.

The researches of Bitter[1] first indicated how modern bacteriological knowledge could explain away certain facts relied on by the localists, or bring them into harmony with the contagionist view of the disease.

Bitter showed that in certain cases the microbe remained confined to the bubo. Such patients were therefore not likely to be a means of transmission of the disease. In other cases, on the other hand, the microbe travelled from the bubo to the blood of the patient, and thence to the excreta, and hence such excreta *à priori* appear liable to transmit the infection.

In 1835 Clot Bey, a French doctor, inoculated himself with pus from a plague bubo[2]. He remained unaffected. This, as shown by Bitter, does not disprove the contagionist doctrine, for it generally happens that at the period of suppuration plague bacilli are no longer present in the pus. In 1802, White, a surgeon serving with the English army in Egypt, inoculated himself from a plague bubo, and died of plague on the seventh or eighth day afterwards. In this case plague bacilli must have been still present if the inoculation was the cause of the illness.

Although the discovery of plague bacilli in the dejecta of patients appears to settle the question of their contagiousness in a positive sense, it may be pointed out that from the practical standpoint, the contagionist view is by no means accepted as regards an established epidemic. In practice it has been found in India that the infected locality is a far greater source of danger than the plague patient.

[1] *Report of the Commission sent by the Egyptian Government to Bombay to study Plague* (Cairo, 1897), p. 64.

[2] Proust, *La défense de l'Europe contre la Peste*, p. 161.

Hence the policy of evacuation, which measure may be regarded as harmonising with the "localist" theory of plague. On the other hand, as regards places not yet infected, the policy adopted is "contagionist." The thing to be feared is the actual importation of the infection by a plague patient or infected person. No one now-a-days would believe that the plague virus originates *de novo* as a miasma bred from an accumulation of filth.

Plague a "miasmatico-contagious" disease.

The question arises whether from the theoretical standpoint also plague should not be regarded from this combined contagionist and localist point of view, whether, in other words, it should not be regarded as belonging to the group of miasmatico-contagious diseases, such as cholera or enteric. In the case of cholera we know that the disease is carried from place to place by human intercourse, if not by infected persons. The microbe is usually present in the dejecta of the patient, but during an outbreak the dejecta are not the usual source of infection. An imported cholera patient is not likely to be the origin of a severe outbreak unless the microbe contained in his dejecta succeeds in reaching the public water-supply. Should this happen isolation of the patients as fast as they occur will not bring the outbreak to an end. It is necessary to evacuate the locality, or at any rate to change or purify the water-supply. Similarly, with plague, the question arises, whether during the outbreak the dejecta of the patient are the chief source of infection, or whether, on the other hand, the microbe in the dejecta is not usually dangerous unless and until it reaches some special "nidus."

Dejecta of human patients not an important source of infection.

It should be recognised that the belief that the dejecta of patients are actually the source of infection, is a deduction drawn from the fact that they frequently contain plague bacilli. It is not an induction based on definite *à posteriori* proofs, except as regards a few cases of apparent infection from sputa in the pneumonic form of the disease, and a few curiously limited outbreaks from this source. It is difficult to accept the view put forward by Bitter that pneumonic cases are of greatest importance in spreading the disease, in view of the fact that the pneumonic form of the disease was unknown in certain outbreaks in the Levant that showed great power of spreading, while it formed

a large proportion of the cases in the Pali and Gujerat plagues that spread over only a very restricted area. In the recent plague in Alexandria the complete absence of known infection from pneumonic cases attracted the attention of observers on the spot.

The view that the plague infection in a house is due to the microbes contained in the dejecta of a patient does not harmonise with the fact that, in certain cases, the infection of others does not take place until a period of from twenty days to four months has elapsed from the time of arrival of the first patient. It is difficult to reconcile this latter fact with the view that infection is simply due to the passage of the microbe from the dejecta of the patient to another person through a crack in the skin. Nor does this view harmonise with the fact that there was a great exodus of actual sufferers from plague from Bombay in December, 1896, to every portion of the Bombay Presidency, while the disease did not commence to spread widely through the different districts until September, 1898, at which time, owing to the stringency of the regulations, it had long been impossible for plague patients to travel, and "suspected persons" were liable in most cases to a certain amount of detention.

It is admitted on all hands that only a portion of the actual exported plague cases, during this earlier period, were discovered, owing to the hostility of the population to plague measures. The following facts may be brought forward as additional proofs of the wide spread of plague cases without a concomitant spread of the plague infection at this time in the Bombay Presidency[1].

Before the discovery of any indigenous cases:

In the Broach	District	23	{cases of imported plague} {were discovered between}			Oct. 1896 and March 1898.
,, Kaira	,,	105	,,	,,	,,	Dec. 1896 and Sept. 1898.
,, Ahmednagar	,,	72	,,	,,	,,	Dec. 1896 and Sept. 1897.
,, Khandesh	,,	37	,,	,,	,,	Dec. 1896 and June 1897.
,, Nassik	,,	47	,,	,,	,,	Dec. 1896 and May 1897.
,, Kolaba	,,	41	,,	,,	,,	Oct. 1896 and Jan. 1897.
,, Hyderabad (Sind)	,,	33	,,	,,	,,	Jan. 1897 and Feb. 1897.

The greater number of these imported cases of plague occurred in December 1896. In the case of Nassik Town, it is recorded that "no less than 26 cases of a virulent type of the disease (these cases showed a mortality of 92·73 per cent.), nearly all in the same quarter of the town (Kajipura and its vicinity), in which the conditions seemed to be particularly favourable for its becoming indigenous, occurred, before

[1] Condon, *The Bombay Plague, being a history of the progress of plague in the Bombay Presidency from September* 1896 *to June* 1899. This important official report contains a vast amount of information. I have made extensive use of it in this and the following paragraphs.

one of the local residents contracted the disease." It is impossible to say how many of the discovered cases in the above list were isolated, or in how many cases their dejecta were disinfected. The point on which stress may be laid is, that from the conditions of the case, the panic among the inhabitants of Bombay, the sudden extension of the disease in that town, &c., it is probable that at least as many exported plague cases were overlooked as were discovered. In these undiscovered cases no attempt at isolation or disinfection was made. The dejecta would fall on the mud floors of native houses: the relatives of the patient would even receive his sputa in their hands, wiping it off on to their clothes or on to the walls or floor of the room, as was most convenient. Nevertheless in the great majority of these instances the relatives remained in these "infected" houses exempt from the disease, in strong contrast to what would have been their fate had they lived in "disinfected" houses in a village in which plague and plague operations were in progress.

Thus at one period of the Bombay outbreak there was exportation, and on a very large scale, of infected persons, without resulting local infection. On the other hand, at later periods of the Bombay outbreak, not only was the disease often exported by persons themselves attacked, but also by persons who either escaped the disease or who were only attacked at a later period.

Persons carrying the infection dangerous in villages, comparatively innocuous in camps.

Experience gained both in India proper and in Garhwal shows that a person carrying the infection is a far greater source of danger if he goes to a village than if he goes to a temporary habitation, such as a sanitary camp. The difference in results does not appear to be altogether due to a stricter supervision in the camps, though it is difficult to estimate exactly how far this factor may be operative. In the case of villages, despite immediate removal of the person bringing the infection, and disinfection of the surroundings (as at Kankhal), the event may be followed, after an interval of weeks and months, by an outbreak of the disease. In the case of camps the importation of the infection is rarely followed by more than one or two cases of the disease, and no prolonged incubation period in the locality has been observed. This appears to be the case even if the inhabitants have entered the camps without any previous disinfection of their clothes or goods, or even where from the necessities of the case the camps have been overcrowded or otherwise in insanitary conditions, and though the time of residence may extend to several months. After persons have resided for ten days in sanitary camps it appears to be safe to permit them, at

all events after disinfection of clothing, to go to other villages. Thus, it is probable that cases of prolonged incubation periods in the locality are not due simply to the microbe having under natural conditions a greater power of resisting desiccation, &c., than it appears to have in the laboratory; on the contrary they appear due to the fact that some condition is present in villages that is favourable to this slow development of the plague infection, which condition is not usually present in temporary camps, or at least not present to so great a degree as in villages. That the condition in villages favourable to the development of plague is the presence of rats is an obvious suggestion. As an apparent proof of its correctness the history of the outbreak in the village of Mahlgahla in the Punjab in the year 1898 may be cited.

Spread of infection independently of human aid.

The infection was introduced by a woman who was attacked by the disease, and who on the day following her attack, with her whole family, was removed from the village to an isolated camp. Twenty-one days later seven of the inhabitants of the village were attacked. All the inhabitants were then put into camp, and the disease apparently came to an end seven days after this had been done. The persons up till then attacked were members of the Chamar caste, and lived in a separate quarter of the village. Four days after the disease had apparently stopped, disinfection of the evacuated quarters of the village in which no human cases of plague had occurred, was commenced. Numbers of dead rats were found in each of these quarters. Consequently there was another outbreak of plague, amounting to 76 attacks, chiefly among persons engaged in disinfection, and the disease only came to an end when disinfection was stopped. Persons disinfecting the Chamar quarters were not attacked, but infection was rife among those who entered other parts of the village in which no human cases, but merely a rat mortality, had occurred. Consequently in this case the infection spread through the village independently of human aid, and as it would appear, at first sight, owing to the agency of rats.

Plague in Kankhal and Jawalapur.

But that this conclusion is not necessarily correct is indicated by the history of plague in the adjoining towns of Kankhal and Jawalapur in the United Provinces[1]. In these cases there can be no doubt that the disease spread through each of the towns independently of human influence. In these towns, as soon as a case occurred, not only the relatives of the patient, but also the inhabitants of the whole of the

[1] See *Evidence before the Indian Plague Commission*, Vol. II. p. 50.

surrounding quarters of the town, were turned out into a sanitary camp. On several occasions, after a block comprising some acres in extent had been evacuated, the next case occurred after an interval of ten to twenty days in a house on the margin of the evacuated area. To the officials on the spot the plague appeared to spread centrifugally from the original infected centre as if carried by some creeping insect. In neither case was any direct evidence obtained that rats played a part in the spread of the disease. In the case of Kankhal, a rat mortality occurred between one and two months before the commencement of the disease among human beings, and during the subsequent disinfection of the town neither living nor dead rats were found. In Jawalapur, on the other hand, no rat mortality was observed, either before or at the commencement of the outbreak among men, and during the disinfection of the town living rats were observed. The outbreak began in January among men, and no mortality among animals was observed till March, when plague was bacteriologically diagnosed in three rats and a few monkeys. The total number of human plague cases in Jawalapur was 116. Of these no less than 29 were of persons or of relatives of persons employed in disinfection, and consequently may be regarded as further proofs of the infection of the locality. Deducting these 29 cases, there remains 87 of the ordinary inhabitants of the town. Out of these 87, no less than 13 were grain dealers, or relatives of grain dealers. Of these 13 cases, 11 had occurred before the end of February, and the remaining two early in March. That is to say they all occurred at the commencement of the outbreak, and formed more than a third of the cases during this period. During the later and longer part of the epidemic the grain dealers remained exempt. What special liability can grain dealers have for plague except that due to the fact that their shops and houses are infested by rats?

Liability of grain dealers to contract infection.

The liability of grain dealers to plague in India has often been observed and quoted as a proof that the spread of the disease is due to rats. But if the validity of this argument is admitted, surely the converse must be true, namely, that an immunity of grain dealers to plague should be regarded as a proof that the spread of the disease is not due to rats. That is to say the facts recorded for the Jawalapur outbreak indicate that at its commencement the spread of the disease

was due to rats, while at a later period its spread was due to some other agency.

Precisely similar facts were noticed during the outbreak in the town of Bombay, though owing to concealment of cases the statistics are less reliable than in the case of Jawalapur. Existing figures however show that at the commencement of the outbreak the grain dealers furnished more cases than the members of all other trades dealing with other provisions than grain taken together. Later, in November and December of 1896, they showed no such preponderating susceptibility.

Migration of rats.

It may be replied that the phenomena observed are simply due to an emigration of infected rats. At the commencement of the outbreak they lived in grain dealers' houses. Later they migrated carrying the infection to other houses. In the case of Bombay there is no doubt that such migration occurred. Further, in nearly every district of the town the plague assumed epidemic form at an interval varying from two to six weeks after the first observed death of rats. But it is not clear why the migrating rats did not carry the infection especially to grain dealers' shops in other quarters of the town. Neither does the suggested relation apply to numerous other outbreaks in the Bombay Presidency where no special liability of grain dealers was observed.

No quantitative relation between rat and human mortality.

Yet another line of argument may be brought to bear on the subject. If two phenomena are causally related, we may expect that a variation in one should be followed by a variation in the other. To apply this rule to the matter under discussion, if the disease among rats was the cause of the disease among men that followed it, in so many instances at least, in Bombay, one would expect that where more rats died more men would be attacked, and *vice versâ*. The case of the grain dealers in Mandvie (the first district of the town attacked) may be cited as a strong argument of this kind. But this was only the case at the commencement of the outbreak. During October in Mandvie rats died in other places than in grain dealers' shops. Children amused themselves by throwing the dead rats out of the windows, and I recollect during this month seeing heaps of dead rats that were about to be destroyed in the streets. No such sight was to be seen

in any other district of the city either then or later on. Dead rats used to be observed in various districts as the disease progressed, but never in such numbers as were seen in Mandvie. But these other districts often suffered far more severely than Mandvie from the plague. The Municipal Commissioner in his report states that vast numbers of rats appeared at the end of December in the northern districts in places where they had never been seen before, and regards their appearance as a cause of the severe outbreaks in the suburban villages. But no proof is given that rats died in any great numbers in these places. I visited several infected villages in these localities during January, and my enquiries elicited no definite proof that dead rats had been observed, though some are stated by Weir to have died in Mahim. The migrating rats seem to have disappeared. This, the Municipal Commissioner states, was due to their having travelled still further in a northerly direction across the bridges and causeways from Bombay Island to the mainland, where he believes them to have been the cause of further village outbreaks. In Worli village (north of Bombay) after the place had been evacuated I saw musk rats, which appear to have remained everywhere unaffected by the pestilence.

Agency of rats of different degrees of influence at different periods of outbreak.

In view of the facts here summarised we are led to the conclusion that if rats played a part in spreading the infection, they must have done so to very different degrees at different periods of the epidemic. A precisely analogous conclusion has been arrived at by Gotschlich from a study of the Alexandria outbreak[1].

Interval between mortality of rats and mortality of men.

That the plague is not conveyed directly (to an appreciable extent) from the dejecta of infected rats to men is proved by the fact already noted that the disease only assumed epidemic form among human beings two to six weeks after the first observed mortality among rats in most of the different districts of the town of Bombay. It is further proved by the fact that the disease continued in most districts among human beings long after the rat mortality had come to an end.

[1] See *Zeitschr. f. Hygiene*, Bd. xxxvi. p. 202.

For instance, the Municipal Commissioner's Report states, "By the middle of March (1897) not a rat was to be seen or heard on Malabar Hill; and yet in ordinary times they infest the whole locality and are constantly appearing or making their presence known. They have never returned, or only such a few as to be unnoticeable (up to 2nd October, 1897). Since last March, on the Ridge, where they were very plentiful, I have never seen a rat, and only a few of the musk tribe remain." But plague among human beings in this locality continued till the middle of April, 1897. The Municipal Commissioner states also that: "By the commencement of December nearly all the rats had disappeared from Mandvie and adjacent quarters of the city, while they were noticed in Kamathipura, Tardeo, and Byculla, in great numbers, many of them being found dead. The bubonic plague followed in their track with unerring regularity." But the plague continued in Mandvie and the neighbouring districts till the month of May. It came practically to an end at the same time in the districts of Tardeo, Byculla, and Kamathipura, though these localities had been so much later infected.

Attachment of infection to locality.

A class of facts tending to prove that plague is not usually due to simple direct infection from the dejecta of human patients is the attachment of the infection to the locality. Both in Jawalapur and Kankhal the cases of plague that occurred among recent arrivals in the segregation camps, did not, except in isolated cases, form foci for the further spread of the disease. On the other hand, during this time, the infection was active and spreading in the locality from which these patients had been brought, as evidenced by the following facts:

"The second area evacuated in Kankhal contained the house of one of the leading men in the town. The house was well-built, and ventilated, and kept scrupulously clean. For various reasons we made an exception in favour of this man, and allowed him and his family to remain in their house. Two members of the family were attacked, although all the surrounding houses had been evacuated. A man was attacked by plague in a house in which a woman was about to be confined. The family of nine persons were segregated in their own house, the surrounding houses being evacuated; five members of the family were attacked. In another block that was evacuated there was a house in which a woman owing to recent confinement could not be moved. She with two attendants were left in the house. Her sister was attacked within a week of the evacuation. An old man and his sister were left in an evacuated area to look after a temple. The sister was attacked with plague two months after the surrounding houses had been evacuated, and after the whole town had been evacuated. After the outbreak had to all appearance ceased, four cases occurred in a family living in a collection of huts separated from the town, which for this reason had not been evacuated. Two chonkidars (watchmen) in evacuated areas were attacked. Large numbers of men employed in disinfecting evacuated houses were attacked, these attacks occurring for some time after the town or village had been evacuated.... There was no recrudescence in

Kankhal, but there were two cases imported from Jawalapur. We got a report when these cases occurred, and made enquiries, and found that the people concerned owned houses in Jawalapur, that within three or four days before they were attacked those houses in Jawalapur had been disinfected, and that, in accordance with the usual custom, a member of the family had been to Jawalapur to be present during the disinfection. He was attacked within three or four days of his return. Those were evacuated houses which were disinfected, and not houses in which cases of plague had occurred. Immediately steps were taken to segregate the people. The people of Kankhal themselves were very anxious that there should be no case of plague, and they gave us information." (Mr Winter's evidence before the Indian Plague Commission, Vol. II., p. 50.) This evidence obtained from Jawalapur and Kankhal is especially valuable, because of the completeness of the organisation that was employed for detecting cases of the disease, and for dealing with the outbreak. Similar evidence as to the infection of the locality in Garhwal will be given in a later paragraph.

Spread of plague not due to infected dejecta of men or rats.

Thus, so far from the patient's dejecta being the main source of infection, known facts indicate that only in a small proportion of instances does the microbe in the dejecta pass into the condition in which it produces infection of human beings in Indian plague. Facts are even compatible with the supposition that this practically never occurs apart from certain pneumonic cases. The problem of the means of the spread of plague here indicated is by no means solved by a reference to rats. Though in some cases there can be no doubt that they play a part in the spread of the disease, other cases that have been brought forward indicate that the plague can spread and remain attached to a locality apart from this agency.

Historical evidence teaches us that the most virulent outbreaks recorded have occurred among populations that habitually wear boots and shoes, rather than among populations that go barefooted. This fact militates against the idea that infection is due to the entry of the microbe through fissures in the skin of the feet. Still less probable is it that this is a usual mode of entry of the microbe for rats, which animals, as we may well believe, but rarely cut their feet by treading on stones or thorns, and are by no means so liable to wounds from other causes as they sometimes are to plague. Laboratory experiments show that the plague microbe loses its infectious power by repeated passages through rats by subcutaneous inoculation. Should these experiments be further substantiated, they would furnish good grounds for doubting whether contagion from rat to rat, or from rat's dejecta to rat, is the

usual means of spread of the disease among these rodents, and to a less extent from rats to men. The apparently spontaneous limitation of outbreaks of pneumonic plague caused by direct infection indicates that the plague microbe may undergo a similar diminution of its infective power by repeated passages through human beings.

Thus it is improbable that the true "nidus" of the plague bacillus is either dirt, or rats, or men; though either of these agents may be concerned in the exportation of the disease from one locality to another, or may be responsible for a few and isolated attacks of the disease, and must, in any event, be regarded as suspect from the practical standpoint.

Infection not due to transference of infected blood by biting insects.

The general immunity to infection of attendants in plague hospitals makes it improbable that bugs and mosquitoes cause human infection by biting while their proboscides are still fouled with the blood of septicaemic patients. It is difficult to see why the proboscis of the flea should be more liable to transmit infection in this way, whether we are dealing with fleas that normally bite human beings, or fleas liberated from infected rats.

Simond[1] has suggested that fleas deposit dejecta at the moment of biting, and that the microbe contained in such dejecta is afterwards accidentally rubbed into the bite, and so causes infection. But if fleas can be dangerous in this way, why should not other biting insects that are present in plague hospitals similarly infect the attendants? Further, it may be doubted whether this theory adequately explains the prolonged incubation period in the locality and persistence of the infection so often observed in outbreaks of plague. In further illustration of this point I will quote in detail a description from Planck[2] of an outbreak in a Garhwali village. The outbreak was due to an importation of the disease, and as already stated such outbreaks in Garhwal are never accompanied or preceded by a mortality among rats according to the careful observations of Planck. We cannot therefore, in this case, invoke a chain of cases among rats to explain the infectivity of the houses which lasted for at least five months.

The facts are as follows: In November, 1876, a boy named Keshrua went to Balt village to fetch wheat for seed, and slept in a house in which was a woman

[1] *Loc. cit.* [2] *Loc. cit.*

suffering from plague, believed then by the villagers to be typhus. He returned to his village Sirar, and fifteen days later his sister was attacked. She died after three days. While she was ill Keshrua was attacked and died. Five days after this death his little brother sickened and died. Ten days later his father was attacked. The man's brother and daughter were attacked within a few days. The village was a comparatively large one, thirty-six houses being shown in the plan in Dr Planck's report. On the occurrence of the above cases of plague the inhabitants fled away to live in temporary huts on the village lands. The survivors of the infected families consisted of Usup Singh's wife and three children. No person of the village would approach them, and they were found living in a grass hut near the infected houses when Dr Planck visited the place on the 28th January. The villagers regarded these persons as doomed though no case of plague had occurred for about two months.

The villagers wished Dr Planck to burn all of the three infected houses that had been inhabited by the two infected families. These were the last three in a terrace of nine houses. A third brother Gunga Ram had however just returned to the village, and he refused to allow his house to be burnt. The other two houses were burnt on the 28th January.

No attacks had occurred during December. Besides burning the houses Dr Planck burnt the clothes and blankets of the remaining women and three children, giving them new clothing instead. A better hut was also provided for them. But on the 30th January the woman, Usup Singh's wife, was attacked and died on the 31st. The new hut in which she died was thereupon burnt ; also all her new clothes and blankets. The new clothes and blankets of the children, and also of an aunt who had been persuaded to live with them were also burnt. A new hut and fresh food and clothing were again provided.

On the 2nd February Gunga Ram's son was attacked, and died after two days' illness. Gunga Ram had promised not to allow anyone to enter his house, but despite his promise he had sent the boy there in the evenings to tie up the cattle in the lower story, which was used as a cattle pen.

On the 8th February the above-mentioned aunt, a girl, and also another child, an infant, were attacked. Owing to the cold they had been unable to bear the misery of life in a hut, and at night time had gone into the cattle pen of Gunga Ram's house, which was no longer occupied by Gunga Ram's cattle since his son had been attacked. The infant died on the 10th, and the aunt on the 13th. The bubo of the girl suppurated and she recovered.

On the 12th of February Gunga Ram was attacked. After his son had died he had ventured to the village to live in a house above the one which had been spared, and which latter he often visited. He died on the 15th.

On the 27th February a woman was attacked. She had been in the habit of tethering her cattle at night in house No. 4 of the infected terrace. She recovered.

On the same date another woman, a widow, was attacked, but recovered. She also had been in the habit of tieing up her cattle at night in the infected terrace at house No. 5.

On the 13th March a young woman was attacked. She had lived in a detached house with the first of the above two women. She died on the 15th.

On or about the 25th March a boy was attacked who lived with his father and

sister in an isolated hut. He had been in the habit of going every day to tether cattle and fetch food from a house just above the infected terrace.

On the 31st March the father of this boy was attacked. Dr Planck does not state whether or not this man went to the village to fetch food for the family when the boy was attacked.

From the end of March the inhabitants carefully avoided the village site and all remained in good health. At the end of May the village was thoroughly cleaned, and early in June it was inhabited without ill effects.

Suggestion that the plague bacillus causes a disease of some species of flea.

The only view of the matter that appears to me likely to lead to an explanation of the facts is that the true "nidus" of the plague infection is some species of flea in which the microbe causes a slowly developing infection that at length renders the insect capable of transmitting the disease, and in which insect the virus can retain or regain its virulence.

Interval between time of reception of infection by flea and development of its power of transmitting the disease.

If Simond's view were true, namely, that the flea merely retains the microbe in its intestine and passes it out with its dejecta, one would expect fleas to be most virulent immediately after, or soon after, ingesting the blood of infected rats. The theory now put forward is that the microbe developes in the flea, and only after a lapse of time is in a position to reach the proboscis in the act of biting. This theory obviously presupposes an interval between the time of reception of the virus by the flea and the development of its capacity to pass this virus on to other animals. As explained above such an interval is usually observed in outbreaks of plague.

Changes in the habits of fleas as the rat population dies off may explain cases in which rats appear to play different parts in the spread of the disease at different periods of the outbreak.

Differences in the habits of fleas in different localities may be the cause of abnormal outbreaks in which certain susceptible species of animals temporarily or permanently escape. The class of facts here referred to, and which have been described in earlier paragraphs, are impossible to explain on the theory that plague transmission is simply a chance passage of the microbe from infected dejecta to accidental cuts or scratches on the bodies of susceptible animals.

Evidence that seasonal variations in plague outbreaks may be connected with habits of fleas.

Changes in the habits of fleas at different periods of the year may be the cause of seasonal variations of plague so frequently observed. The following facts may be quoted in support of this suggestion. In the spring of 1901 I was studying fleas obtained from cats and dogs in Agra, and found no difficulty in obtaining as many of these insects as I required. But at the commencement of the hot weather I found that the fleas on the cats living in my laboratory had suddenly and completely vanished. Recognising that the matter was of interest and wishing to subject it to an adequate test I at once offered a half-day's wages to my servants for every flea they could catch. Though the servants lived in different parts of the town and in surrounding villages not a single flea could they produce. A restricted plague epidemic that had been going on in Agra suddenly came to an end at the time of the disappearance of the fleas. On a previous occasion when plague was present in Agra (A.D. 1618), it appears to have come to an end at the same time of the year[1]. Tidswell noticed an analogous coincidence in Sydney[2]. He says, "It happened that whereas during the prevalence of plague, we had no particular difficulty in collecting the 100 specimens (of fleas) mentioned above, yet since the disappearance of the epizootic the rats examined have been remarkably free from fleas. Our frequent searches for specimens have been most usually fruitless. It was only now and then that we have come across two or three fleas on some particular rat. These rare specimens were either *Typhlopsylla musculi*, *Pulex fasciatus*, or *Pulex pallidus*." That is to say the fourth species of flea that Tidswell found on rats, the *Pulex serraticeps*, had vanished at the time of the cessation of plague in Sydney. It is noteworthy that this species of flea has been found in Sydney on cats, dogs, rats, human beings, and a wallaby.

J. Ashburton Thompson[3] from a study of the 1902 plague outbreak

[1] The Emperor Jehangir in his diary thus refers to this outbreak in Agra:—"During the last three years the disease has caused many deaths during the winter; but at the beginning of the summer it dies down to reappear at the first commencement of the cold weather." Jehangir mentions the mortality among rats. I am indebted for this reference to Colonel Lukis of the Indian Medical Service.

[2] "Report on Ectoparasites of the Rat," by Dr F. Tidswell, published in Ashburton Thompson's *Report on a Second Outbreak of Plague at Sydney in* 1902, p. 71.

[3] Ashburton Thompson's *Report on a Second Outbreak of Plague at Sydney in* 1902, p. 78.

in Sydney concludes that the flea must be able to communicate the virus "many hours, and even some days after it has received it." The facts brought forward in this paper suggest that in India the flea may retain the power of transmitting the disease for weeks or even months. Simond[1] has suggested that the retention of the infection by fleas may be the cause of recrudescences of the disease, which, as he shows, usually occur at the interval of a year after the first appearance of the outbreak.

Evidence of development of plague bacillus within body of flea.

As evidence that the plague microbe developes within the body of the flea, I can only quote the following observation of my own made during the above-mentioned plague outbreak in Agra. In April, 1901, a rat was brought to me that had been found dead in the grain dealers' quarter in Agra shortly after the first human case of the disease had occurred. No trace of the plague microbe could be found, either by microscopical examination or by culture in any of the tissues of the rat. On the rat, however, I found a living flea. This I caught and placed in a tube of sterile bouillon. The tube was violently shaken. The flea was then taken out and placed in a second tube of bouillon and similarly treated. The process was repeated several times, with the object of removing as far as possible saprophytic bacteria that might be present on the surface of the flea. After the above treatment the flea was dissected, with strict aseptic precautions, under a dissecting microscope. The stomach was taken out and cut in two pieces. One half was placed on agar-agar, and from it a pure culture of plague was obtained (as shown by involution forms on salt agar, &c.). The other half of the stomach was subjected to microscopic examination. The only microbes visible were bacilli with rounded ends identical in appearance with those of plague. These were arranged in clusters of about a dozen individuals each, and appeared to be embedded in the tissues of the stomach wall. No bacilli were observed in the liquid contents of the stomach. The arrangement of the bacilli in clusters obviously suggests that they were engaged in reproduction *in situ*.

Previous work on the subject.

Ogata (1897)[2] first found plague bacilli in fleas and suggested that these insects might be concerned in the spread of the disease.

The German Bombay Plague Commission (1897) found plague

[1] *Loc. cit.*

[2] "Ueber die Pestepidemie in Formosa." *Centralbl. f. Bacteriol.*, Vol. xxi. 1897, pp. 769–777.

bacilli in fleas, but did not consider that the bite of the flea was the means of transmission of the malady.

Simond[1] in 1898 observed bacilli identical in appearance with those of plague in the stomach contents o fleas from infected animals. He brought forward evidence tending to prove that such infected fleas could transmit the infection by biting. He also adduced epidemiological reasons for believing that this was the most frequent means of transmission of the disease.

In the same year, on epidemiological grounds, I suggested that some biting insect might be the means of the transmission of the disease from rats to men[2].

Nuttall[3], who has subjected earlier work on the relation of fleas to plague to a critical examination, suggests that possibly rat-fleas may attack men in plague epidemics, when their natural hosts are dying off rapidly in and about human dwellings (see Addendum, Note II.).

Thompson and Tidswell[4] in Australia have found that fleas from plague rats, when triturated and injected into susceptible animals could produce plague.

Galli-Valerio[5] has attacked the theory that plague can be conveyed from rats to men by fleas on the grounds that species of fleas found on rats do not bite men.

But Thompson in Australia and Tiraboschi[6] in Italy have since found *Pulex serraticeps* on rats, and it is known that this species of flea will bite man.

Tidswell has shown that three species of flea found on rats will bite man, namely, *P. serraticeps*, *P. fasciatus*, and *P. pallidus*.

Failure has frequently accompanied attempts to transmit plague to healthy rats by means of fleas coming from infected animals, as in the

[1] *Loc. cit.*

[2] Hankin, "La propagation de la Peste," *Annales de l'Institut Pasteur*, 1898, p. 705.

[3] Nuttall, "On the rôle of insects, arachnids, and myriapods as carriers in the spread of bacterial and parasitic diseases of man and animals. A critical and historical study." *Johns Hopkins Hospital Reports*, Vol. VIII. 1899, p. 21.

[4] Thompson and Tidswell, *Report on the Outbreak of Plague at Sydney*, 1900.

[5] Galli-Valerio, "Quelques observations sur la morphologie du Bacterium Pestis et sur la transmission de la Peste bubonique par les puces des rats et des souris." *Centralbl. f. Bacteriol.*, Vol. XXVIII. 1900, p. 842.

"The part played by the fleas of rats and mice in the transmission of Bubonic Plague," *Journal of Tropical Medicine*, Feb. 1902.

"Les nouvelles recherches sur l'action des puces des rats et des souris dans la transmission de la Peste bubonique." *Centralbl. f. Bacteriol.*, Vol. XXXIII. 1903, p. 753.

[6] Tiraboschi, "Beitrag zur Kenntniss der Pestepidemiologie. Ratten, Mäuse, und ihre Ektoparasiten." *Archiv für Hygiene*, Vol. XLVI. p. 251.

experiments of Tidswell[1] and Kolle[2]. Simond only succeeded in two experiments out of four. Simond has suggested that such failures are due to the fact that healthy rats can catch fleas. But in view of the considerations brought forward in this paper it is obvious that such experiments should be repeated, firstly, with different species of flea, secondly, regard being had to the possibility that the fleas may only become capable of transmitting the disease after the lapse of a period varying from ten to twenty days or more from the date of their exposure to the infection.

Chalmers, the Medical Officer of Health, in his Report on Plague in Glasgow in 1900 has adverted to the possibility of an insect (the flea) "acting as a temporary host of the bacillus of plague," and inoculating the disease by puncture, "after the manner in which the malarial parasite is transmitted through the bite of certain mosquitoes."

I owe my best thanks to Dr Nuttall, of Cambridge, for his advice and help while writing the latter portion of this paper.

ADDENDUM.

I. In "The Blot upon the Brain," by Dr W. W. Ireland (published in 1893), on p. 125, the author in the course of an attempt to prove the insanity of Mahommed Tughlak, states "We read of a great pestilence in 1341, well-nigh destroying a whole army in the Deccan. Was this an invading epidemic of the Black Death which so terribly thinned the population of Europe a few years later?" This remark of Dr Ireland's has only come to my knowledge after the first correction of my proofs.

II. I have just seen a newspaper report of a paper by Captain Liston, I.M.S., on the subject of "Plague, rats, and fleas," which contains important evidence bearing on the above views. The report is published in the *Times of India* for the 26th November 1904, and the paper will be published in full in the *Journal of the Bombay Natural History Society*. Captain Liston brings forward definite evidence of the truth of Nuttall's suggestion that rat-fleas may attack men after their natural hosts have died off. He finds that man rarely harbours the rat-flea under normal conditions. Out of 246 fleas caught on man, in the absence of plague, only one was a rat-flea. But during a plague outbreak in a lodging house (apparently in Bombay), out of 30 fleas caught on the human inhabitants no less than 14 were rat-fleas. Similar observations were made during a plague epidemic among guinea-pigs. Under normal conditions these animals are not infested by rat-fleas. But during the epidemic no less than 18 rat-fleas were found on one sick guinea-pig. Captain Liston also urges the particular infectivity of infected houses at night as "proof that the plague virus is transmitted by means of fleas." We at length seem to be in a position to understand the psalmist's phrase "the pestilence that walketh in darkness." The report of Captain Liston's paper only came into my possession on the 14th December, long after my own paper was sent to press.

[1] *Loc. cit.*

[2] Kolle, "Bericht über die Thätigkeit in der zu Studien über Pest eingerichteten Station des Instituts für Infectionskrankheiten." *Zeitschr. f. Hygiene*, Vol. xxxvi. 1901, p. 397.

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